



Interactive
Comment

Interactive comment on “A dasymetric data supported earthquake disaster loss quick assessment method for emergency response in China” by J. Xu et al.

Anonymous Referee #1

Received and published: 18 March 2015

GENERAL COMMENTS

The manuscript describes the validation of a procedure aimed at estimating the human loss due to earthquakes in China. The manuscript also focuses on how the availability of good dasymetric maps of population and building area allows to improve the speed, accuracy and positioning of loss estimation. The method is based on two of phases. In the first one, depending on the different level of expected earthquake intensity, a set of pre-calculated maps of loss are built. The second phase concerns the time period immediately after an event: a new map is defined, where the intensity is modeled exploiting information on the magnitude and the epicenter location of the real earthquake.

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The loss is then conveniently calculated, given the estimated intensities, querying the pre-calculated set of maps of loss. The method is validated using 4 different earthquakes and proves to work enough well though it is not able to predict the loss due to other types of geological phenomena triggered by the earthquakes (e.g. the landslides).

The paper doesn't present new data or concepts but the method is, as far as I know, interesting and relevant within the scope of NHESS.

While the method is described enough clearly there is an important element that is not discussed enough. The method is based on the dasymetric map concept but in the manuscript there is not a clear description of the procedure adopted for the creation of the dasymetric maps of the buildings and population. I believe that the revised version of the paper should include, at least, a section where the production of these maps is described. This is important also for the reproducibility of the method.

The abstract is enough complete but the title could be improved. The font sizes of the texts inside the figures seems too much small. Previous works are correctly cited but at least in one case, the corresponding manuscript is not immediately available from the web. The overall presentation of the manuscript is easy to understand and the length is adequate. However in the "specific comments" section I describe some points that must be clarified.

Though I'm not a native English speaker, I think that the language is not enough good and fluent.

SPECIFIC COMMENTS

In the paper the authors frequently use the phrase: "km grid format". This can be rephrased simply to "raster format" or to "raster format having a resolution of ...". This is particularly true if they have used a geographical projection and a resolution of 30". As the authors say the equivalence of 30" to 1 km is not true for all the latitudes. More in

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general it could be technically interesting to know exactly the type of projection adopted for the maps and the project in general..

p1476 r20: Here the authors cite a former work but there is no reference and it is not possible, for a reader, to know how the dasymetric maps have been created. This is fundamental also for the reproducibility of the method.

p1479 r17: As above. Moreover the figure 7 is not discussed enough and it is not easy to understand for a reader.

p1479 r26. Again, it is not clear which method and data the authors have used to create the dasymetric maps.

p1483 r2: it is not clear what the j stands for.

p1485 r3-6: I understand the concept but it is not completely clear. I think the authors should rephrase with something as: “1) The DPM tables have been associated with the vector map of the earthquake zoning of China. 2) The vector map has been then converted to a raster map where the cells values depend on the DPM tables. The raster resolution is the same of the exposure maps”

p1485 r3 and r8 : I’m not sure that “gridding” is the most convenient word. I suggest “vector-to-raster conversion”

p1485 r10-13: I understand but it is not clearly written. Please rephrase.

P1485 r18: Here ND is directly proportional to f_t but, according to table 4, f_t is inversely proportional to the earthquake intensity. Is it normal? Unfortunately I wasn’t able to check reading the manuscript of Ma and Xie (2000) since it is not available online.

P1486 r3: What is the “map algorithm method”? Perhaps “map algebra”?

1486 r3-5: Not clear. The authors say that “ND” depends on “RD” which in turn depends on “RB”. However RB was estimated, (section 4.1.3) using 100 maps, so now,

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IMHO. we should have $100 \times 10 = 1000$ maps.

P1487 r3-5: Which is the approach when an earthquake hits this dividing zone.

P1487 r20-22 Please add more details. Not enough clear.

P1487 r8-12. I can't understand.

TECHNICAL CORRECTIONS

P1489 r23: "it still not easy", please add the missing "IS".

Please check this references.

In the text: Nadim et al 2003 (or 2004) ? Lin, 2011 (or Lin (et al.), 2011) (GB/T18208.4, 2005 (is not in the references list)

In the references section: Dobson, J. E., Bright, E. A., Coleman, P. R., Durfee, R. C., and Worley, B. A.: LandScan: a global population database for estimating populations at risk, Photogramm. Eng. Rem. S., 66, 849–857, 2000 (not in the text)

Whitman, R. V., Reed, J. W., and Hong, S. T.: Earthquake damage probability matrices, available at: www.iitk.ac.in/nicee/wcee/article/5_vol2_2531.pdf (last access: 20 November 2014), 1973 (not in the text)

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 1473, 2015.

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